

Proposal Reviews

#237: EVALUATION OF MERCURY TRANSFORMATIONS AND TROPHIC TRANSFER IN THE SAN FRANCISCO BAY/DELTA: IDENTIFYING CRITICAL PROCESSES FOR THE ECOSYSTEM RESTORATION PROGRAM

US Geological Survey

Initial Selection Panel Review

Research and Restoration Technical Panel Review

Delta Regional Review

Sacramento Regional Review

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Prior Performance/Next Phase Funding

Budget

Initial Selection Panel Review:

CALFED Bay-Delta 2002 ERP PSP Initial Selection Panel Review

Proposal Number: 237

Applicant Organization: US Geological Survey

Proposal Title: EVALUATION OF MERCURY TRANSFORMATIONS AND TROPHIC TRANSFER IN THE SAN FRANCISCO BAY/DELTA: IDENTIFYING CRITICAL PROCESSES FOR THE ECOSYSTEM RESTORATION PROGRAM

Please provide an overall evaluation rating.

Explanation of Recommendation Categories: Fund

- **As Is** (a proposal recommended for funding as proposed)
- **In Part** (a proposal for which partial funding is recommended for selected project phases or components)
- **With Conditions** (a proposal for which funds are recommended if the applicant contractually agrees to meet the specified conditions)

Consider as Directed Action in Annual Workplan (a proposal addressing a high priority action that requires some revision followed by additional review prior to being recommended for funding)

Not Recommended (a proposal not currently recommended for funding-after revision may be considered in the future)

Note on "Amount":

For proposals recommended as Fund As Is, Fund In Part or Fund With Conditions, the dollar amount is the amount recommended by the Selection Panel.

For proposals recommended as Consider as Directed Action in Annual Workplan, the dollar amount is the amount requested by the applicant(s).

Fund	
As Is	X
In Part	-
With Conditions	-
Consider as Directed Action	-
Not Recommended	-

Amount: **\$2,262,567**

Conditions, if any, of approval (if there are no conditions, please put "None"):

None

Provide a brief explanation of your rating:

This project will examine processes related to biogeochemical transformations and transfers of mercury among physical (sediment and water) and biotic (food web) compartments at two sites, Franks Tract (a 3188-acre lake in the central Delta) and the Cosumnes River. The processes to be studied include methylmercury production and degradation, transfer of methylmercury across the sediment-water interface, entry of methylmercury into the base of the food web (very poorly understood but critically important), and food-web transfer and biomagnification. The Panel believes that this information will advance scientific understanding of pathways leading to methylmercury contamination of aquatic biota in the Bay-Delta system. The project goals are ambitious, but the likelihood of successful completion should be high, given the substantial knowledge, skill, and experience of the investigators. The budget is realistic for an effort of this magnitude.

To increase the strategic benefit of the information emanating from this research to CALFED, the Selection Panel strongly encourages the principal investigators to strengthen the linkage of this research effort to ecological restoration activities. The selection of study sites for this project was science-driven, and not linked to ecological restoration per se. The Panel strongly recommends that the investigators incorporate modifications, such as the inclusion of one or more sites being influenced by ecological restoration (e.g., the Yolo Bypass), to directly address management concerns (particularly multi-regional priority MR-5) related to ecological restoration in this mercury-contaminated ecosystem.

Research and Restoration Technical Panel Review:

CALFED Bay-Delta 2002 ERP PSP Research and Restoration Technical Panel Review Form

Proposal Number: 237

Applicant Organization: US Geological Survey

Proposal Title: EVALUATION OF MERCURY TRANSFORMATIONS AND TROPHIC TRANSFER IN THE SAN FRANCISCO BAY/DELTA: IDENTIFYING CRITICAL PROCESSES FOR THE ECOSYSTEM RESTORATION PROGRAM

Review:

Please provide an overall evaluation summary rating:

Superior: outstanding in all respects;

Above Average: Quality proposal, medium or high regional value, and no significant administrative concerns;

Adequate: No serious deficiencies, no significant regional impediments, and no significant administrative concerns;

Not Recommended: Serious deficiencies, significant regional impediments or significant administrative concerns.

Overall Evaluation Summary Rating	Provide a brief explanation of your summary rating
-Superior	The panel generally agreed that the PIs in this project are experts in geochemical cycling of trace metals and more than worthy of an undertaking. The proposal however, at times reads as four different subproposals contained in a single package. Two reviewers questioned the objectives in Task 3 as being somewhat outside of the true goal of assessment and process-level work at the site. While it is an interesting phase of study, the \$0.5 million devoted to this work at such early phase of this project may be premature. If preliminary food web work shows a strong benthic component, then additional funds may be used for this purpose. It will be quite a chore to achieve everything discussed in the proposal, but the emphasis on processes and their portability to other systems is the real key. The PIs should be more in tune with management concerns and might have addressed possible management scenarios in their project development plan.
XAbove average	
-Adequate	
-Not recommended	

1. **Goals and Justification.** Does the proposal present a clear statement of goals, objectives and hypotheses? Does the proposal present a clear justification and conceptual model for the project?

The PIs present a detailed proposal aimed at investigating processes leading to bioaccumulation of methyl Hg in the food web of selected sites in the Bay-Delta region. They propose to study a reflooded delta farmland area and a key free-flowing tributary. The goals are clearly stated and the research is hypothesis driven. Bioaccumulation of MeHg in the region is a pressing concern and this proposal addresses key mechanisms.

Knowledge of the types of processes described by the PIs is certainly warranted. In order to evaluate MeHg in this ecosystem, it is imperative to describe the rates of production and formation. Uncertainties exist in how MeHg bioaccumulates in all aquatic systems, particularly with regards to speciation (S, DOC, Fe), bacterial habitat, process rates, and biotic uptake. This proposal addresses all of these aspects, and includes relevant modeling (speciation and bioenergetics). Food web structure of the Bay-Delta area must be addressed prior to evaluating Hg concentrations in fish. The PIs propose to use some state-of-the-art methods to describe the processes.

2. **Likelihood of Success (Approach, Feasibility, Capabilities and Performance Measures).** Is the project likely to succeed based on the approach, feasibility and project team capabilities? Are the proposed performance measures adequate for measuring the project's success?

The approach of this proposal appeared to be four separate studies that were occurring in common sites. Although a conceptual model is presented, the work did not appear to be well integrated, with the exception of some of the pore water chemistry and the methylation/demethylation studies proposed. Given the experience of the proposed research team, it should have been a more coherent proposal with succinct interrelationships, without having 23 pages of pertinent data as appendices. Since this proposal is not well integrated, one must question whether the goals of individual researchers will take precedent over the translation of results for management concerns. The information that will be gathered by this research is vital to managers in the basin and a little more thought to integrating the results and translating them to management concerns. A workshop to distill our collective knowledge on Hg into an integrated conceptual model and practical guide appears to be an afterthought rather than a design for the study.

From reading reports and other proposals dealing with Hg in the basin, it appears that tributary sources, especially during high flow periods, are major sources of MeHg to the system. The key study sites have been chosen based on very little preliminary methylation/demethylation data and the main characteristics of comparison are vegetated vs. non-vegetated sites. It would appear that a detailed study of Franks Tract might include a mass balance approach. If tributary flow during flooding periods is important, one must measure the input of particulate matter to the system.

One of the strengths of this project is the food web component, particularly stable isotopic links with MeHg bioaccumulation. The authors indicate that food chain length may affect bioaccumulation and that is an area that has not been studied that widely in Hg studies. This type of research is certainly warranted. The submerged vegetation hypothesis was generated with such scant data on methylation rates in this system. Are there other biological processes that affect methylation in these floodplains.

Task 3 seems a bit out of place with respect to the assessment work proposed in the other phases of the project. While this is an interesting sidelight of the study, it does not directly address assessment of the site and may be a bit premature at this point. If benthic processes prove important from food web studies, this phase of the research might be better warranted.

The approach is fully documented, but a reviewer is still left to wonder what the final products will be. Individual projects will probably prove successful, but they appear somewhat disjointed. More detail should be paid to event-based sampling and assessment. From a management perspective, better results and model inputs might be better described from more detailed monitoring of processes. While the uptake studies are interesting, they might be better delayed until the major fluxes and uptake routes are identified.

3. **Outcomes and Products.** Will the project advance the state of scientific knowledge in general and/or make an important contribution to the state of knowledge of the Bay-Delta Watershed? For restoration proposals, is the project likely to contribute to ecosystem restoration or species recoveries in a significant way? Will the project produce products useful to decision-makers and scientists?

From a truly academic research standpoint, there is no doubt that these investigators will produce top-notch scientific publications from their proposed research. They have a proven track record in the peer-reviewed literature and have been successful in many environmental projects in the past. Our main concern is what will be the direct benefits from a management perspective from their study? Unless the major aspects of this study are linked and the results condensed in more than a conceptual model, the interpretative outputs for management concerns are doubtful. It is imperative that the various investigators work together in the Delta system, particularly between those doing process-oriented work and those doing mostly monitoring. Ideally, an over-arching conceptual model will result from the various research groups.

As stated above, the PIs are experts in the field of mercury cycling and well-qualified to perform the work described. Most have the infrastructure to complete the work, although there is request for supplemental equipment.

4. **Cost/Benefit Comments.** Is the budget reasonable and adequate for the work proposed?

There is no doubt that this is an expensive project dedicated to a few research sites, but most of the cost is due to the high costs of analyses. The main issue for budgetary consideration is whether such a large project will pay dividends for management concerns. Perhaps some of the funding for more expensive lab-related studies (\$0.5 million) such as Hg radioisotopic phytoplankton uptake studies might be better spent hiring a modeler who could tie together the field results into a more coherent interpretation for management applications.

5. **Regional Review.** How did the regional panel(s) rank the proposal (High, Medium, Low)? Did the regional panel(s) identify significant benefits (regional priorities, linkages with other activities, local involvement) or impediments (local constraints, conflicts with other activities, lack of local involvement) to this proposal? What were they?

Delta Regional Review - High Project looks like it will provide good information on Hg methylation processes in the Delta. Well-prepared proposal that includes good science and thoughtful approach. Useful for future modeling. Committee had some question of the need for such detailed knowledge of Hg transformations at this stage. Before being funded, this work should be clearly tied in to previous CalFed Hg directed action studies.

Sacramento Regional Review Medium While this would be an interesting scientific exercise it was not apparent how this would aid the Sacramento River watershed in restoring salmonid habitat. While it is understood that mercury is a problem in the ecosystem it is not seen as a large threat to salmonids. In general, the panel referred to the tech panels on how all the mercury proposals fit in with current efforts related to mercury.

6. **Administrative Review.** Were there significant concerns about the proposal with regard to the prior performance, environmental compliance and budget administrative reviews? What were they?

Applicant must clarify amount requested from CALFED based on state or federal fund source. Budget tables appear to be assuming a federal fund source.

Miscellaneous comments:

None

Delta Regional Review:

Proposal Number: 237

Proposal Title: EVALUATION OF MERCURY TRANSFORMATIONS AND TROPHIC TRANSFER IN THE SAN FRANCISCO BAY/DELTA: IDENTIFYING CRITICAL PROCESSES FOR THE ECOSYSTEM RESTORATION PROGRAM

Overall Ranking: -Low -Medium **XHigh**

Provide a brief summary explanation of the committee's ranking:

Project looks like it will provide good information on Hg methylation processes in the Delta. Well-prepared proposal that includes good science and thoughtful approach. Useful for future modeling.

1. Is the project feasible based on local constraints?

XYes -No

How?

Hydrologic conditions have been considered and flexibility built into schedule. PIs have experience field sampling in the Delta area.

2. Does the project pursue the restoration priorities applicable to the region as outlined in the PSP?

XYes -No

How?

PSP specifically cites a need to better understand processes that determine Hg methylation in the Delta and tributaries and the impacts of floodplain creation on water quality and fish (e.g. enhancing Hg uptake by flooding). Cosumnes River is of particular interest.

3. Is the project adequately linked with other restoration activities in the region, such as ongoing implementation projects and regional planning efforts?

XYes -No

How?

Project has support of Cosumnes River Project. Project recognizes other Hg-related work in the Delta, but has no formal ties or agreements. Project intends to fill knowledge gaps identified in current local Hg research. Fish collected in collaboration with UCD and DWR studies.

4. Does the project adequately involve local people and institutions?

XYes -No

How?

The PIs are not strictly local, but the project has the support of the Cosumnes River Project.

Other Comments:

Strong science is apparent in this proposal. Committee had some question of the need for such detailed knowledge of Hg transformations at this stage (ie without a modeling framework).

Before being funded, this work should be clearly tied in to previous CalFed Hg directed action studies.

Sacramento Regional Review:

Proposal Number: 237

Applicant Organization: US Geological Survey

Proposal Title: EVALUATION OF MERCURY TRANSFORMATIONS AND TROPHIC TRANSFER IN THE SAN FRANCISCO BAY/DELTA: IDENTIFYING CRITICAL PROCESSES FOR THE ECOSYSTEM RESTORATION PROGRAM

Overall Ranking: -Low **XMedium** -High

Provide a brief summary explanation of the committee's ranking:

While this would be an interesting scientific exercise it was not apparent how this would aid the Sacramento River watershed in restoring salmonid habitat.

1. Is the project feasible based on local constraints?

XYes -No

How?

They have been sampling in these areas and have access. They are proposing to use techniques that are proven.

2. Does the project pursue the restoration priorities applicable to the region as outlined in the PSP?

XYes -No

How?

It could be construed to apply to PSP priorities 1 and 3 but does not clearly fit these. Priority 7 does include Hg studies within it.

3. Is the project adequately linked with other restoration activities in the region, such as ongoing implementation projects and regional planning efforts?

XYes -No

How?

It does fall within the Mercury Study efforts but that is only loosely linked with other restoration activities.

4. Does the project adequately involve local people and institutions?

-Yes **XNo**

How?

The proposaa does not involve any local watershed groups.

Other Comments:

While it is understood that mercury is a problem in the ecosystem it is not seen as a large threat to salmonids.

In general, the panel referred to the tech panels on how all the mercury proposals fit in with current efforts related to mercury.

External Scientific: #1

Research and Restoration External Scientific Review Form

Proposal Number: 237

Applicant Organization: US Geological Survey

Proposal Title: **EVALUATION OF MERCURY TRANSFORMATIONS AND TROPHIC TRANSFER IN THE SAN FRANCISCO BAY/DELTA: IDENTIFYING CRITICAL PROCESSES FOR THE ECOSYSTEM RESTORATION PROGRAM**

Conflict of Interest Statements:

I have no financial interest in this proposal.

XCorrect

-Incorrect

In the blank below please explain any connection to proposal, to applicant, co-applicant or subcontractor or to submitting institution (write "none" if no connection):

None

Review:

Please provide an overall evaluation summary rating:

Excellent: outstanding in all respects;

Good: quality but some deficiencies;

Poor: serious deficiencies.

Overall Evaluation Summary Rating	Provide a brief explanation of your summary rating
XExcellent	This appears to be an exceptionally well thought out and detailed proposal to investigate the mechanisms of mercury transformations and trophic transfer in the SF Bay/Delta and will answer some very important questions regarding mechanisms underlying and driving these processes.
-Good	
-Poor	

1. **Goals.** Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the concept timely and important?

Rating: Excellent

While this proposal falls somewhat outside my expertise, a compelling case is made for this effort.

2. **Justification.** Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Rating: Excellent

The proposed work is a logical extension of previous work done under CALFED, and such a study examining the mechanisms of mercury transformations and trophic transfer are fully justified based on the relative lack of information currently available. The conceptual model put forth is very well designed to address the hypotheses posed.

3. **Approach.** Is the approach well designed and appropriate for meeting the objectives of the project? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology or approaches? Will the information ultimately be useful to decision-makers?

Excellent on all counts.

4. **Feasibility.** Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives?

Excellent

The approach is eminently feasible, and has a high likelihood of success. All parameters to be measured are fully documented and detailed and the research team that has been formed to perform the work is top notch.

5. **Project-Specific Performance Measures.** Does the project include appropriate performance measures to measure success relative to the project's goals and objectives? Is there enough detail as to how the performance measures will be quantified? For restoration projects, are monitoring plans explicit and detailed enough to determine if performance measures will be adequately assessed?

Rating: Excellent

6. **Products.** Are products of value likely from the project? Specifically for restoration projects, are products of value also likely from the monitoring component? Are interpretative outcomes likely from the project?

Rating: Excellent

7. **Capabilities.** What is the track record of applicants in terms of past projects? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Rating: Excellent

Members of the research team have done funded work under CALFED before and are all well known in their respective fields.

8. **Cost/Benefit Comments.** Is the budget reasonable and adequate for the work proposed?

Rating: Excellent

All aspects of the budget are very well detailed and quite reasonable, once it is understood that each task involves multiple personnel. Note that there is considerable costsharing involved in this proposal, with USGS contrib.. \$131K, U of Md 272K, and SUNY 69K over the proposal period.

Miscellaneous comments:

External Scientific: #2

Research and Restoration External Scientific Review Form

Proposal Number: 237

Applicant Organization: US Geological Survey

Proposal Title: **EVALUATION OF MERCURY TRANSFORMATIONS AND TROPHIC TRANSFER IN THE SAN FRANCISCO BAY/DELTA: IDENTIFYING CRITICAL PROCESSES FOR THE ECOSYSTEM RESTORATION PROGRAM**

Conflict of Interest Statements:

I have no financial interest in this proposal.

XCorrect

-Incorrect

In the blank below please explain any connection to proposal, to applicant, co-applicant or subcontractor or to submitting institution (write "none" if no connection):

I have worked with Robert Mason on Lake Michigan and currently at ELA in Canada. I have worked with Mark Marvin in the Everglades. Neither interaction should affect my ability to review this proposal in an unbiased manner.

Review:

Please provide an overall evaluation summary rating:

Excellent: outstanding in all respects;

Good: quality but some deficiencies;

Poor: serious deficiencies.

Overall Evaluation Summary Rating	Provide a brief explanation of your summary rating
-Excellent	While the PIs in this project are experts in geochemical cycling of trace metals, the proposal reads as four different subproposals wrapped in a single package. In other proposals reviewed, goals and tasks are integrated with many PIs and it is clear to see the interrelationships. That is not the case with this proposal. If this proposal is not well designed, I worry that the goal of integration with separate projects in the Bay-Delta region will not be a smooth process. The PIs should give more thought to the influence of extreme seasonal events and to a true ability to model the results of the separate sub-projects.
XGood	
-Poor	

1. **Goals.** Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the concept timely and important?

The PIs present a detailed proposal aimed at investigating processes leading to bioaccumulation of methyl Hg in the food web of selected sites in the Bay-Delta region. They propose to study a reflooded delta farmland area and a key free-flowing tributary. The goals

are clearly stated and the research is hypothesis driven. Bioaccumulation of MeHg in the region is a pressing concern and this proposal addresses key mechanisms.

2. **Justification.** Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Knowledge of the types of processes described by the PIs is certainly warranted. In order to evaluate MeHg in this ecosystem, it is imperative to describe the rates of production and formation. Food web structure of the Bay-Delta area must be addressed prior to evaluating Hg concentrations in fish. The PIs propose to use some state-of-the-art methods to describe the processes.

3. **Approach.** Is the approach well designed and appropriate for meeting the objectives of the project? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology or approaches? Will the information ultimately be useful to decision-makers?

I had a difficult time with the approach of this proposal. In a way, it seemed like there were four separate studies that were occurring in common sites. The work did not appear to be well integrated, with the exception of some of the pore water chemistry and the methylation/demethylation studies proposed. Given the experience of the proposed research team, I would have expected a more coherent proposal. The information that will be gathered by this research is vital to managers in the basin and a little more thought to integrating the results, more than a workshop to distill our collective knowledge on Hg into an integrated conceptual model and practical guide appears to be an afterthought rather than a design for the study.

From reading reports and other proposals dealing with Hg in the basin, it is my understanding that tributary sources, especially during high flow periods, are the major sources of MeHg to the system. The current study design and the sampling plan in Table 1 appear strongly inadequate to address the dynamic nature of this estuary system. The key study sites have been chosen based on very little preliminary data and the main characteristics of comparison are vegetated vs. non-vegetated sites. It would appear that a detailed study of Franks Tract would include a detailed mass balance. If tributary flow during flooding periods is important, one must measure the input of particulate matter to the system. Methylation rates and MeHg release may be totally decoupled if the source of MeHg was from dissolution of recently deposited particulate matter. Without a mass balance to a system, it would be difficult to use this study site as a predictor for effects establishment of wetlands on MeHg cycling. Similarly, processes in the Consumnes floodplain may be dependent on one event per year. Studying these sites on a limited basis may miss key inputs.

One of the strengths of this project lies in the application of stable isotopic tracing of the food web with regard to MeHg bioaccumulation. The authors indicate that food chain length may affect bioaccumulation and that is an area that has not been studied that widely in Hg studies. This type of research is certainly warranted.

I was also surprised by the detail to which the submerged vegetation hypothesis was generated with such scant data on methylation rates in this system. Are there other biological processes that affect methylation in these floodplains? It was my recollection that the floodplains and the tidal marshes in the area are quite productive with respect to plankton and periphyton. Are complex planktonic-bacterial mats a part of these systems and if so, what is their impact? Studying uptake processes on pure cultures may be informative, but the complex interactions of

bacteria and plankton in these mats may be better understood together. Might field mesocosms in these systems and stable isotopic spikes be a better approach? This has appeared to be an important approach for the Florida Everglades.

4. **Feasibility.** Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives?

The approach is fully documented, but I am still left to wonder what the final products will be. Individual projects will probably prove successful, but they appear somewhat disjointed. More detail should be paid to event-based sampling and assessment. From a management perspective, better results and model inputs might be better described from more detailed monitoring of processes. While the uptake studies are interesting, they might be better delayed until the major fluxes and uptake routes are identified.

5. **Project-Specific Performance Measures.** Does the project include appropriate performance measures to measure success relative to the project's goals and objectives? Is there enough detail as to how the performance measures will be quantified? For restoration projects, are monitoring plans explicit and detailed enough to determine if performance measures will be adequately assessed?

I am not sure what specific measures are identified to achieve success of the project. At the first level in Figure 10, topics for publications are listed as the first level of success. That seems a bit backwards. Specific levels of success might be to a) describe major fluxes in and out of the system; b) assess most important factor regulating methylation at Franks tract, etc. For restoration purposes, it would be important to address the most important factors in design of wetlands that would prohibit methylation.

6. **Products.** Are products of value likely from the project? Specifically for restoration projects, are products of value also likely from the monitoring component? Are interpretative outcomes likely from the project?

From a truly academic research standpoint, I have no doubt that these investigators will produce top-notch scientific publications from their proposed research. They have a proven track record in the peer-reviewed literature and have been successful in many environmental projects in the past. My main concern is what will be the direct benefits from a management perspective from their study? Unless the major aspects of this study are linked and the results condensed in more than a conceptual model, the interpretative outputs for management concerns are doubtful.

7. **Capabilities.** What is the track record of applicants in terms of past projects? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

As stated above, the PIs are experts in the field of mercury cycling and well-qualified to perform the work described. Most have the infrastructure to complete the work, although there is request for supplemental equipment.

8. **Cost/Benefit Comments.** Is the budget reasonable and adequate for the work proposed?

There is no doubt that this is an expensive project dedicated to a few research sites. I am a little concerned about the choice of field sites being made from a very limited set of methylation data. Perhaps some of the funding for more expensive lab-related studies such as radioisotopic uptake studies might be better spent hiring a modeler who could tie together the field results into a more coherent interpretation.

Miscellaneous comments:

External Scientific: #3

Research and Restoration External Scientific Review Form

Proposal Number: 237

Applicant Organization: US Geological Survey

Proposal Title: **EVALUATION OF MERCURY TRANSFORMATIONS AND TROPHIC TRANSFER IN THE SAN FRANCISCO BAY/DELTA: IDENTIFYING CRITICAL PROCESSES FOR THE ECOSYSTEM RESTORATION PROGRAM**

Conflict of Interest Statements:

I have no financial interest in this proposal.

XCorrect

-Incorrect

In the blank below please explain any connection to proposal, to applicant, co-applicant or subcontractor or to submitting institution (write "none" if no connection):

none

Review:

Please provide an overall evaluation summary rating:

Excellent: outstanding in all respects;

Good: quality but some deficiencies;

Poor: serious deficiencies.

Overall Evaluation Summary Rating	Provide a brief explanation of your summary rating
XExcellent	Overall, I thought that this was an excellent proposal. While perhaps it is not strong on the monitoring component, its process-oriented perspective will tie in well with other studies. The study directly addresses most of the "burning" issues in Hg cycling today, including demethylation, sediment cycling, speciation, lower food web bioaccumulation, and use of novel isotopic techniques. It will be quite a chore to achieve everything discussed in the proposal, but the emphasis on processes and their portability to other systems will make its attempt worthwhile.
-Good	
-Poor	

1. Goals. Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the concept timely and important?

The project goals and objectives are very clearly presented, and tie in directly to stated hypotheses. The hypotheses are extremely relevant to this and other aquatic systems, and work towards filling research gaps identified by CALFED. Most of the points to be examined are timely questions, particularly the focus on speciation, bioaccumulation in the lower food web, and the role of sediments/water fluxes. This project attempts to study contrasting systems from the full spectrum of process and spatial scales.

2. **Justification.** Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Uncertainties exist in how MeHg bioaccumulates in all aquatic systems, particularly with regards to speciation (S, DOC, Fe), bacterial habitat, process rates, and biotic uptake. This proposal addresses all of these aspects, and includes relevant modeling (speciation and bioenergetics). Particularly attractive is work done on the sediment/water interface, to establish controls on MeHg production and flux. Demethylation, both microbial and photochemical, are very important, relatively unconstrained processes that may significantly alter MeHg exposure to biota. The conceptual model (Hg cycle) is clearly presented, and relates well to the tasks proposed.

3. **Approach.** Is the approach well designed and appropriate for meeting the objectives of the project? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology or approaches? Will the information ultimately be useful to decision-makers?

The general approach is to examine virtually all aspects of Hg biogeochemistry in two locations of contrasting MeHg in biota. The alternative would have been a monitoring approach consisting of many sampling sites, but few process activities. The authors have chosen an ideal framework for attacking the problem, including some monitoring, a great deal of process-oriented work, complex uptake and speciation studies, and modeling exercises. The work proposed is no small feat, but a system as complex as the Bay Delta likely requires such magnitude. I particularly like the "nuts and bolts" perspective that they are taking with regards to sedimentary processing and uptake of MeHg. Such findings will likely be applicable to many other aquatic systems beyond the Delta.

4. **Feasibility.** Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives?

It appears that the authors are well equipped to do the proposed research, as no major equipment purchases are included. Further, a relatively large amount of the simpling will be done in-house by students and staff, which reduces costly consulting services--more bang for the buck. The authors have had much experience in the fields of Hg biogeochemistry, food web ecology, and the Bay Delta system in general. I believe that the liklihood is high for successful completion of the listed tasks, and addressing hypotheses and research gaps. The scale of the proposal is large, but reasonable given the hypotheses and complexity of the system. Those involved would be kept quite busy.

5. **Project-Specific Performance Measures.** Does the project include appropriate performance measures to measure success relative to the project's goals and objectives? Is there enough detail as to how the performance measures will be quantified? For restoration projects, are monitoring plans explicit and detailed enough to determine if performance measures will be adequately assessed?

The authors have provided the standard array of performance measures, including peer reviewed publications, meetings, and other forms of outreach. Details on the quality assurance plan for various analyses would be helpful. Quarterly and annual reports would assure that the work is progressing as it should.

6. **Products.** Are products of value likely from the project? Specifically for restoration projects, are products of value also likely from the monitoring component? Are interpretative outcomes likely from the project?

Ultimately, dissemination of the findings to managers, the general public, and other Bay-area investigators is the most important goal. The authors indicate that this transfer of knowledge is a priority. It is imperative that the various investigators work together in the Delta system, particularly between those doing process-oriented work and those doing mostly monitoring. Ideally, an over-arching conceptual model will result from the various research groups.

7. **Capabilities.** What is the track record of applicants in terms of past projects? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

The qualifications of the PI's are excellent, and well-poised to manage a project of this scale. Expertise is evident in the areas of mercury dynamics, analysis, and food web dynamics. There are no weak links here.

8. **Cost/Benefit Comments.** Is the budget reasonable and adequate for the work proposed?

The benefits of in-house analysis are obvious, and the costs related to supplies and salary are reasonable. While this is a new project, most of the infrastructure is already in place to begin the study.

Miscellaneous comments:

Prior Performance/Next Phase Funding:

New Proposal Number: 237

New Proposal Title: EVALUATION OF MERCURY TRANSFORMATIONS AND TROPHIC TRANSFER IN THE SAN FRANCISCO BAY/DELTA: IDENTIFYING CRITICAL PROCESSES FOR THE ECOSYSTEM RESTORATION PROGRAM

1. Prior CALFED project numbers, titles, and programs: *(list only projects for which you are the contract manager)*

ERP 01-N20 - Transport Transformation and Effects of Se and C in the Delta: Implications for ERP

2. Prior CVPIA project numbers, titles, and programs: *(list only projects for which you are the contract manager)*

N/A

3. Have negotiations about contracts or contract amendments with this applicant proceeded smoothly, without persistent difficulties related to standard contract terms and conditions?

-Yes -No -N/A

If no, please explain any difficulties:

Interagency agreement was negotiated between CALFED-USGS. NFWF was not involved in contract negotiation, only implementation.

4. Are the status, progress, and accomplishments of the applicant's current CALFED or CVPIA project(s) accurately stated?

XYes -No -N/A

If no, please explain any inaccuracies:

Contract 01-N20, Transport, Transformation, and Effects of Se and Carbon in the Delta of the Sacramento-San Joaquin Rivers: Implications for ecosystem restoration, commenced Fall 2001.

Project is in initial phase.

5. Is the applicant's progress towards these project(s)' milestones and outcomes to date satisfactory?

XYes -No -N/A

If no, please explain deficiencies:

Contract 01-N20, Transport, Transformation, and Effects of Se and Carbon in the Delta of the Sacramento-San Joaquin Rivers: Implications for ecosystem restoration, commenced Fall 2001.

Project is in initial phase.

6. Is the applicant's reporting, records keeping, and financial management of these projects satisfactory?

XYes -No -N/A

If no, please explain deficiencies:

Invoicing has not commenced.

7. Will the project(s) be ready for next phase funding in 2002, based on its current progress and expenditure rates?

-Yes -No -N/A

If no, please explain:

Contract 01-N20, Transport, Transformation, and Effects of Se and Carbon in the Delta of the Sacramento-San Joaquin Rivers: Implications for ecosystem restoration, commenced Fall 2001.

Other Comments:

Budget:

Proposal Number: 237

Applicant Organization: US Geological Survey

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1. Does the proposal include a detailed budget for each year of requested support?

☒Yes ☐No

If no, please explain:

2. Does the proposal include a detailed budget for each task identified?

☒Yes ☐No

If no, please explain:

3. Does the proposal clearly state the type of expenses encompassed in indirect rates or overhead costs?

☒Yes ☐No

If no, please explain:

4. Are appropriate project management costs clearly identified?

☒Yes ☐No

If no, please explain:

5. Do the total funds requested (Form I, Question 17A) equal the combined total annual costs in the budget summary?

☐Yes ☒No

If no, please explain (for example, are costs to be reimbursed by cost share funds included in the budget summary).

Applicant must clarify amount requested from CALFED based on state or federal fund source. Budget tables appear to be assuming a federal fund source.

6. Does the budget justification adequately explain major expenses?

☒Yes ☐No

If no, please explain:

7. Are there other budget issues that warrant consideration?

-Yes ☒No

If yes, please explain:

Other Comments: